ORAL HISTORY 2 TRANSCRIPT

HENRY O. POHL
INTERVIEWED BY SUMMER CHICK BERGEN
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BERGEN: Today is March 26, 1999. This oral history interview with Henry Pohl is being conducted at the offices of the Signal Corporation in Houston, Texas, for the Johnson Space Center Oral History Project. The interviewer is Summer Chick Bergen, assisted by Tim Farrell and Kevin Rusnak.

We're glad to have you with us again.

POHL: Thank you. Good to be here.

BERGEN: Last time we talked about your early career up basically through Apollo, and you brought us a film that you had made when you were working on rockets.

POHL: Right.

BERGEN: Why don't you tell us a little bit about that film.

POHL: Okay. That was back in the early sixties or late fifties, I guess, [19]'59 or '60, when I made that, and that's when we started getting into clustering rockets. Matter of fact, we had just gotten authority from DARPA [Defense Advanced Research Projects Agency], I believe it was, to cluster eight Jupiters together. That was our original intent, was to take eight Jupiters and tie them all together and launch them eight at a time for the first stage. Later on, we kind of changed that up a little bit and made it a more of a unitized construction.

But we didn't know back then much about base heating. We had Polaris, had four nozzles on it, solid rocket motor, and we lost several of those because the exhaust would impinge and go back up on the back of the rocket and overheat the base of the rocket, and basically it would explode. So I got the job of developing some model rockets, small ones, 600 pounds of thrust at 600-pound chamber pressure, and clustering eight of those little rockets in a thirteen-inch circle, diameter. That was one-twentieth-scale model of the Saturn IB. And then we took those and put them in the wind tunnels up at Tullahoma [Tennessee].

At different mach numbers and different altitude, different pressures in the accel, we could duplicate the conditions that you would see in flight. We put calorimeters all in the base and thermocouples in the base, and in that way we could develop the criteria that we needed for base heating. We later used that same model to design the flame deflectors at the Cape [Canaveral, Florida] and in Huntsville [Alabama], where we did the static testing of the Saturn IB.

That was kind of a fun project. You know, I'd never designed a rocket engine, didn't know very much about them, and hadn't been out of school but maybe four years at that time when Mr. Haukohl came in and asked me to design an engine that's one-twentieth the size of the H1 engine. And I checked out all of the books that I could find. Sutton was about the best one, and I'd go home, and I'd stay up till two o'clock in the morning studying those things and playing with them, and I finally designed one, but I was not a very good draftsman. So I kind of sketched it out and took it down to a draftsman there to put it in the proper format for the shop to build, and he almost laughed me out of the room. He said, "You've got to think rotary. You've got to think rotary."

I said, "What's rotary?"

He says, "It goes around and around." He says, "We can't make anything unless it's round." Well, that was because we had those engine lathes, and, you know, they did rotary work, and I had all kinds of octagon-shaped surfaces on the thing because I wanted it all to fit together where it would fit in the cluster nice and neat.

The very first one we built—well, I took the prints over to the shop, and pretty late that afternoon I got a call from the shop telling me that they couldn't make it, that they only had fifty-three twenty-eight-thousandths-diameter drills to drill the little holes in the injector, they had already broken forty of them, and they only had three holes in there. So I went and told Mr. Haukohl, "They can't make my injector over at the shop."

Well, he called the German that was in charge of the shop, a guy by the name of Fritz Vandersee [phonetic], and he came down there and he looked at those prints. He said, "Oh, that's a piece of cake. Who says they can't do it?"

I says, "Hamby."

He says, "Oh, Hamby don't know what he's doing." So we got in his old O.D. Chevrolet, pulled out to the shop, walked in there, and he started telling them, "Get this and get that and get something else," and he went to a great big old drill press he had there and chucked a little motor in that thing that was air-driven, and ran it at about 40,000 rpm or something like that, really, really fast, and then he chucked those little drills in there and put a bunch of lead on this thing so he could balance it. He'd stick one of those eyepieces and catch it up there, a magnifying thing, and look at that thing, and he'd take his finger. He punched three or four holes in that thing just like he's punching holes in butter.

When he got through, he told Hamby, he says, "You stay here tonight until you learn how to do that, and then in the morning, you teach the machinist how to do it." That was a neat experience. Now, that man had been down that road once before and he knew how to do those kind of things. But we made it.

The very first one that I designed, it ran for about twenty seconds, several tests about twenty seconds long, before I finally burned it out. It kept running hotter and hotter and hotter, and it finally burned out. You know, we built about four or five more going back and using those same prints, and they would burn out within one and a half or two or four seconds. Almost instantly, fire would shoot out through the sides of them. So I had to give up on fuel cooling, I just didn't have enough fuel to cool it with fuel, and water-cooled them.

Now, when you water-cool them, that meant I had two more lines going into each chamber. I had water in and water out, and along with the ignition lines, the chamber correction lines, the water lines, the fuel lines, the LOX [Liquid Oxygen] lines, it got pretty busy back in there. You could see that most of that design was kind of cut and try, you know. If we were packaging all that stuff, we would put a line in the middle, and then we'd run a line on outside of that, and just kept building it up from the inside out until we got all the plumbing and all the instrumentation and everything in there. And it worked out real good. Matter of fact, we run over 200 tests up at Tullahoma in the wind tunnel on it in a very short period of time, never had any real problems with it at all.

Anyway, [Wernher] von Braun wanted a movie to take to Congress to show Congress. So I took the mostly test film—we had cameras on every test we were in, there in Huntsville then, sometimes two or three cameras taking pictures, and then we'd go back and study the film and try to figure out what was going on. So I took mostly test film, and I'd go home at night and I'd sit there, took one of these little editing things home, and I'd spool it back and forth and cut and—back then you'd cut it and glued it together, glued the film together with a cutter.

And then I got the camera crew one day, and we went around the test lab there taking pictures of people, just kind of at random, you know, find somebody and take a picture of them at a desk working on some drawing or something. Like I got Fritz Pauly [phonetic] in there. He was another one of the Germans that worked on tiger tanks in Germany. But we put together that film, and when I got it the way I wanted it, then I took a tape recorder and I described what was going on as I was viewing the film and sometimes had to put a little filler in there, and sometimes I had to cut a little out to get it to fit. But then they took that, and they got somebody else then, they typed all of that out and got somebody else down to read the script. Of course, they didn't like my voice, the way I talked.

Von Braun then took that up to Congress and showed it to members of Congress, and it went over so big, they liked it so much, that they came back and made a copy of it, the movie, and gave it to me. I've still got the movie. It's a 16-millimeter movie, and I've still got the little government sheet that went with it giving me permanent—it still belongs to the government, but I've got permanent retention of it. You know, I can keep it. And I've been saving that because I didn't want somebody to say I took some government property sometime.

But that was a fun project. Of course, I worked day and night on the thing, and we did that. From the time I got the instructions to do that until we had made that movie, it was less than a year, nine or ten months, that period of time. So I'd run a test during the day and then look at the data, just a quick look at the data, make some changes, just sketch them out on a tablet, stop by the shop with the changes that I wanted to make, leave the drawings there that night, and next morning when I came to work, I'd stop by the shop and pick up the part. They'd make it overnight, and I'd take it down there, and the technicians would put it in the test stand, we'd run another test, I'd make some changes on it, take it back to the shop, and they would make that part that night. The next day we'd put it back in there, run another test.

So things moved very fast then. We didn't have a lot of supervision, we didn't have a lot of people telling us we couldn't do this, we couldn't do that, or you've got to do it this way, you've got to do it that way. Everything needed to be done yesterday, and they wanted it yesterday. So we made a lot of progress, and I was proud of it because the dad-gummed thing worked.

That was a really good training for when I came down here, then, when I moved down to the Manned Spacecraft Center [later Johnson Space Center, Houston, Texas] and they put me in charge of overseeing the development of the reaction control rockets for

Apollo and for Gemini. I probably had more experience with designing and building those small rockets than anybody else in the country at that time because I had done it all myself. I didn't farm this out to somebody else and that out to somebody else. There was nobody else to farm it out to. I did all the analysis, I did all the drawing, everything on it. So that's kind of the history of that project.

BERGEN: You did so much work on the development of those early rockets that eventually led to the development of the Saturn V, and then you came and worked on Apollo spacecraft. Did you ever get to watch a Saturn V launch?

POHL: Yes. We went down. I took the kids and my wife down to the Cape for, I believe it was Apollo 14. That was the flight after Apollo 13, and it was kind of an iffy thing to make sure that it went off on time, but we went down there and got to watch it lift off. And it's kind of agonizing to sit there and watch that thing take off, because it just barely would get off the ground, and it moved ever so slow for a long time. It was not until it got way up in the sky that it started moving fast. So you was just sitting there just hearing that rumble and that bouncing in your chest from the acoustics from it, and you just think it was just never, never going to get out of sight. But that was good experience. I enjoyed seeing that one.

And I saw quite a few of the Jupiters early on and some of the Redstones launched from down there for the military. As a matter of fact, I've got a 35-millimeter slide someplace around there of a Jupiter. It looks like it was taken from the top right straight down, but it wasn't. It turned over and turned over right straight toward us. My buddy and I was down there on a beach down there about five miles from that thing or four miles from it, I guess, to watch the launch, and he had a 35-millimeter camera on a tripod, and I had a movie camera, but my movie camera, I couldn't see anything on it. It didn't have enough focal length on it. But he took one picture when it lit off, and the next one, it was coming right straight at us, and the third one is a big ball of fire. It just did a ground loop. When it took off, the engine went hard over because of a failure in a circuit board. A solder joint in the circuit board caused it to go hard over, and it blew up, and I guess blew up about halfway between us and where it was launched. There were just two of us out there on the beach at night.

BERGEN: It must have been exciting watching those early rockets launch.

POHL: You know, in that day—people can't visualize that now. Most of that was classified then, but if they got out of sight, it was a success. The criteria for the launch to be successful was that it got out of sight before it blew up. It didn't have to get to its target. If it got out of sight—and a lot of them didn't.

BERGEN: We've come a long way since then.

POHL: You know, the progress that we made in rocket engine development and missile development between about '58 and '62 was phenomenal. In the late fifties, I believe one year we had fifteen Thors to blow up. Not many people remember Titan I, but they must have built about sixteen Titan Is, and I am not sure that any of those ever got away from the pad. Maybe one. They lost all their test facilities in Denver [Colorado], and they shipped two of them down to the Cape to be launched that had never been static-fired, and both of those blew up, so they lost all of their launch facilities down there, so now they didn't have any more launch facilities, and it was going to take a while to rebuild those. So they made a proposal to change from LOX/kerosene to storables, Aerozene program 50 and N_2O_4, which was self-igniting propellants.

Titan I was one of the most complex, complicated rocket engines I have ever seen in my life. You know, it had nine valves in it, control valves. Any place where you wanted to control a function, they did it with nine control valves, three in series and then three strings of that so that they could get the timing right and the reliability out of them time after time. So it was triple redundant on those things, and they still had all kinds of problems with them.

They went to Titan II, and Aerojet [General Engineering Corporation] went from one of the most complex rocket engines that was ever developed to the simplest. Titan II, that [unclear]-storable engine, was very, very simple, very straightforward, and turned out to be a very, very reliable engine, and they still use that engine today in the Titans.

BERGEN: In the sixties, while you were working on Apollo, actually the Shuttle development began. When did you begin in your branch—you were in the Dynamic Systems Branch?

POHL: Right.

BERGEN: When did you begin work on the Space Shuttle?

POHL: Well, I sent some guys over to work with Jim [James A.] Chamberlin in the very beginning, when Max [Maxime A.] Faget formed that little team to start looking at a winged vehicle to replace the Saturn class of vehicles. They formed a little team, it was kind of in secret, went over to one of those buildings out there and started working on that. That was a good experience, too, because I sent one guy by the name of Darrell Kendrick [phonetic] over there, and he was over there about three days, and Mr. Chamberlin called me one day and says, "I've got a problem with your Mr. Kendrick."

I said, "What's the problem?"

"He balked."

I said, "What do you mean he balked?"

He said, "You know what happens to a mule when they balk. They won't go." [Laughter]

So I called Kendrick, got him on the phone, and I didn't let him know that Mr. Chamberlin had called me, I just was asking him how things were going over there and what he was doing. He says, "Oh, they don't know what they want." He says, "I designed them an RCS [Reaction Control System] system, and then they decided that wasn't what they wanted, they wanted something else. So I designed them another RCS system, and they decided that's not what they wanted, they wanted something else. I just decided I'm going to wait until they make up their mind what they want and then I'll design them a system."

Well, what he didn't understand was that you have to put the whole rocket together and see where it punches out. You know, it's going to get too heavy, or it's not going to fit, something's not going to fit, you're not going to have enough volume, and when that happens, you look at the overall plan, and then you change your criteria a little bit, and then you redesign to this new set of criteria, and you see where it punches out again, and then you change the criteria again, and you just keep changing until you get things to fit in the envelope and volume and the weight. And that's what Kendrick didn't understand. He thought they didn't know what they wanted, and so he was going to wait until they figured out what they wanted before he designed them another RCS system. I got to explaining that to Darrell, that, you know, you've got to keep on doing that, it's an iterative process, and you keep on iterating until you get it to fit.

But they did. They came up with a neat vehicle that was a straight-wing vehicle, and it would have been probably a lighter design than the one we came up with, with the delta-wing vehicle, but the problem with Max's straight-winged vehicle is it didn't fit the Air Force's criteria. The Air Force wanted, I believe it was 900 miles cross range, you know, so that they could land it, they could put it over here and then land it over there someplace, and they wanted to be able to launch anytime and land anytime, and it had to have a lot of cross range.

Well, I think Max's straight-winged vehicle was limited to about 500, 400 or 500 miles cross range, which the way that we turned out using it would have been adequate for what we wanted it for, but it didn't meet the Air Force criteria, and so it went through a lot of iterations getting to that. Then Max came up with swing engines, which is a—Max and Caldwell [C. Johnson] were innovators. You know, they were always innovating, inventing something, and what they wanted to do to get the CG [center of gravity] right on the airplane was, after you launched it and it was in orbit and got the payload out of the payload bay, they wanted to take the engines and unhook them and take the engines and swing them back over and put them in the payload bay and close the payload bay doors, and that way they'd have all of the weight, the CG, up forward and not have to worry about the intratemperatures on the engine. But he was never able to sell that.

BERGEN: What kind of new challenges did this development in the Space Shuttle bring to your group?

POHL: Well, in the propulsion area, you know, we pushed those main engines very, very hard. That's a very high-performance engine, very high chamber pressure for that day and time, very, very light weight for the thrust that they were producing. I would say that we came out with that program in the only time in the history of this country when it would have been successful. If we would have waited another two years or another three years before we started development on Shuttle, we probably would not have been able to do it, and the reason I say that is that the people that designed the main engine on Shuttle were the same ones that designed the Redstone engine, the Jupiter engine, the Thor engines, the Atlas engines, the H1, the F1, and the J2. So they had gone through seven—that same group of people, in their working lifetime, had designed and built seven different engines before they started the Shuttle development.

Now, a lot of those people retired, and a lot of them didn't finish the Shuttle Program, but at least they were the ones that got it started. They knew what was important, they knew what you had to worry about. They had developed the stress calculations and thermal calculations and everything, so they knew what was important, what you had to worry about. If we'd waited another two or three years, those people would have all been gone, and we would have had to learn all over again on the engine development.

That's been one of the problems in this country. You know, we spend a lot of money on a lot of things, but on rocket engines we overbuilt the propulsion community in the late fifties and early sixties, and then we just let it die. It's always been in spurts, and all of the engines, all of the hardware we use now—I guess the Shuttle engine was the last engine that was developed in this country, and that was started in about [19]'72, somewheres in that time frame.

BERGEN: Were you involved with the auxiliary power units?

POHL: APUs? Yes.

BERGEN: Will you tell us about those?

POHL: Well, you know, on Shuttle, we started out with four systems. We had dual tandem actuators, and we had four APUs in there, but the vehicle was like 170,000 pounds, and they wanted to get it down to about 150,000 pounds, so we went with three, and single actuaries with switching valves on the actuaries. That was a tough one to sell, because the reliability people didn't like it, the safety people didn't like it, because it had too many single-point failures in it. Well, when you get right down to it, all the structures are single-point failures. You know, a structure breaks and you've lost the vehicle. So that didn't bother me too much.

The thing that was a little bit disconcerting is that we had not a great deal of experience with hydrozene-running turbines, and we didn't have a great deal of experience with operating these things in zero G. You know, in an automobile, the oil goes down in the sump in the bottom. You can put a pump in the bottom and you keep pumping the oil out because as it goes through the system, it runs back in that sump. Well, in zero G there is no bottom, and that oil just goes where it's slung out to. So we had to design it so that the gears would keep taking the oil and keep pushing it back into a container where the oil pump could pick it up and send it back through the system. That created a few problems, but we got that under control pretty quick.

The gas generator, we had a few problems with getting the catalyst to last, to get some duration out of the catalyst, and we went through some very innovative ways of making the cat [catalyst] beds so that they would expand and contract. When the catalyst got hot it would expand, so to keep from crushing it, you had to let that whole thing expand and come back together.

I guess the one thing that I will never, never forget, though, is when we landed with two APUs burning, and, you know, if somebody would have told me that we would have two failures on the same flight exactly identical to each other, I would have said, "You're crazy. It can't happen in a thousand years," but it did. We had those very, very thin tubes from the valve bracket to the cat bed to keep the heat soak back from getting up and heating the hydrozene too much in the valves on that thing, and they were very thin-walled tubes, and two of them broke on one flight. We had gone through all of that testing and hundreds and hundreds of hours of testing on those things. We had them down here in our vacuum chamber and everything, and never had a problem with them.

Well, it turned out what happened is we were landing the vehicle in California and then piggy-backing it down to the Cape, and what happens when he'd get up at that altitude, he'd kind of pull a vacuum on that whole area because the pressure would go down, and then when you'd land at Florida, you'd come back in, and that humid, moist air and that humidity, the moisture was getting back up in there and reacting with the residual hydrozene in there and causing a hydrate that was very, very corrosive. You had stress corrosion in those tubes that caused the tubes to break.

We had one other problem that we found fairly late. We brought one of those units down here and put it in our vacuum chamber down here, and that was kind of hard to justify. It cost some money, and Sun Strand [phonetic], the builder of that unit, didn't have altitude facilities, so what they would do is pull a vacuum on the exhaust duct on that thing so that would give you the same conditions you had in space as far as performance was concerned.

But we got one, we put it in our vacuum chamber down here in the test area at JSC [Johnson Space Center], and the first time we shut it down, it blew up. Now, we're getting pretty close to flying that thing, and that's a major, major, major catastrophe, and, of course, the program office just absolutely knew that we screwed up the test, we did something wrong in the test.

So we got another unit, brought it down, put it in the test lab. This time we had all the Sun Strand engineers down here and all the Rockwell [International] engineers and our engineers down here, and everybody's looking over everybody's shoulder to make sure that we didn't screw up the test, and it blew up again. It turned out there was one thing that we hadn't thought about, and that is, when we had this whole APU insulated because it ran pretty hot, and we had these shelves that we had built wired on around that thing to keep it from radiating too much heat out.

Well, when you tested it on the ground, that insulation would act like a chimney. The heat would go out the top and cold air would come in the bottom of it, and it would cool it off real quick so it didn't get overheated. Well, when we shut it down in a vacuum, there was no air to cool it, and the heat just had to soak out, and the heat would soak out and soak out into the valves and get the hydrozene that was in the valve hot enough to where the valve would detonate, the hydrozene would detonate in the valve and blow up the valve.

So we had to go through a big fix on that and verify that fix on it. We wound up spraying a little water on the back of it when we shut down to—that was the quick fix—to keep it from getting too hot when it got up to a certain temperature. We'd pulse a little water back there, and in a vacuum that water would instantly vaporize and give you a lot of cooling for just very little water. That's the way we fixed that one.

Those were some of the fun kinds of things that you ought to think about, but it's just not natural for people to think about the absence of gravity, and it's not natural for you to think about the different thermal conditions that you get in a vacuum from what you have in one atmosphere. We live in a gravity all the time, and all of our experiences are based on what you see and feel and hear, and if you can't experience that environment, then you don't watch for the right kinds of things.

BERGEN: Seems like at times testing of the things that had been developed was as much of an engineering challenge as the development.

POHL: A lot of times the facilities required to duplicate the conditions of space is more difficult to construct and operate than the hardware that you're putting on the vehicle, because you've got to think about so many things in the development of those facilities. You know, to try to get a vacuum is not easy. You know, you can get a vacuum up to 100,000 feet or 150,000 feet altitude real easy, but getting from there on down to almost a perfect vacuum is very, very difficult. You've got to resort to Crowell [phonetic] pumping and things like that to get the pressure all the way down, and those facilities are very expensive.

Now, you take somebody that's under contract to build something unique like the APU was, they didn't have those facilities, and for the government to finance building those facilities at that time would have been prohibitive, and we were fortunate here. You know, most people don't realize it, but the people that laid out the Manned Spacecraft Center, they were good. They came from Langley [Research Center, Hampton, Virginia], and every one of them had been used to doing something themselves. You know, they all had kind of the same kind of experience that I had, you know, you design things, you tested things, you broke things, you fixed things. And they put these facilities in down here not because we had lots of money and we could afford it, but Dr. [Robert R.] Gilruth and Dr. Faget and Bob [Robert F.] Thompson and those people knew that we had to train a large number of people, and you can't get that out of a book. You've got to train them by doing it.

So we built those facilities, and we put those facilities in which were the best, very best, that we could build at that time with the technologies that we had, not because we could afford it, but because we had to have it. You had to test your theories, you had to test your thoughts, your ideas, and we found so many things with those facilities that would have just gone undetected.

Take the lunar module [LM], for example. We put the lunar module in that big old vacuum chamber over there. We got cold walls on one side to duplicate deep space. We got these lamps on the other side to duplicate the sun. And most people don't realize it, when you're in space, the side that's facing deep space is minus 250 degrees, the side that's facing the sun is plus 250 degrees. When you see an astronaut running around out there, you've got to realize that one side of him is seeing a temperature of 250 degrees, the other side of him is seeing a minus 250 degrees, that's a 500-degree differential, and humans live around 70 degrees. So that's the reason we've got all these water tubes in there, to run the water around and around and around. It takes the cold from the back and pulls it out to the hot side and the heat from the hot side back to the cold side so they stay at a fairly normal temperature.

But we put the lunar module in that vacuum chamber, and it was literally tearing itself up. The side that was getting cold was coming down. The side that was hot was going up. So the stresses was getting so high in the thing that it was actually breaking it. That's when we came up with this aluminized or goldized mylar that you see all on the bottom stage of that thing. We didn't have that on there and had no intentions of covering it until we ran into the stress problem, because one side was getting too hot, the other side was getting too cold, and nobody thought about that thermal problem.

That reminds me of another deal where I got the job one time to design the valve for the gas generator on the H1 engine. We had separate valves in that thing, and they were sticking, and the timing wasn't right. We were burning out turbine blades. Mr. Haukohl came in there one day and said, "Henry, why don't you design us a valve that's mechanically [unclear]," and I did, and took the design over and presented it to a whole bunch of these people. This one old German, when I finished, says, "I want to thank you for a lot of hard work and a very innovative design." He says, "There is but one problem with it: it won't work."

Well, I was well enough to know, when he said it won't work—he says, "You're going to run kerosene through this side that's made out of aluminum that's so many centimeters thick and you're going to run kerosene through this side over here, and that's going to keep it at roughly 70 degrees, and you're going to run liquid oxygen in on this side over here, and that's going to take it down to minus 298 degrees." And he says, "This side's going to shrink so much, and this side's going to stay up. You've got to have such and such clearance on those shafts, and it's going to warp it enough where the pockets are going to bind in there, and it's not going to work." He said, "You know, maybe not all is lost." He got up and went up there and says, "I think you can take a saw and run a saw right down through here, down so close to the face of it down here, and that might just let this side come down flat and let this side stay up and," he says, "it might work."

Well, Rocketdyne took that design and built it, made a few changes to it and built it, and we never had the first problem with that thing. Every one of them worked, and I can guarantee you, if that German wouldn't have been in there at that time, it wouldn't have worked. We would put it on a test stand, you know, and run a hundred tests on the thing, probably using water to simulate the oxidize and maybe kerosene on the fuel side, and it would have worked like a charm. Then you put it on an engine and you test it on an engine with LOX in there, and it would start all right because it would have been kind of warm when you first started it, but then when you'd try to shut it down, the valves wouldn't close, the valves would have stuck open on the thing. And we would have gave up on it. But the fact that he spotted that right off the bat and thought about that, he'd been down that road before. That wasn't the first time he ran into that problem. And that's something people frequently forget, that the way that you learn is by doing it and by making mistakes.

You know, we've gotten into a situation now where—and it started with Gene's [Eugene F. Kranz] famous comment that failure is not an option. We're not allowed to fail anymore. Well, if you're not allowed to fail, then, by definition, you cannot succeed. I tell everybody I remember the answers to all of the problems I got wrong in college, and I don't even remember the questions on the ones I got right. That's something that young engineers have to have, the flexibility to do some experimentation, to test their theories, to test their ideas and their thoughts, and give them confidence in their ability to do something. We've lost that. We don't allow that anymore, for fear of failure, for fear that they're going to do something that's going to break or something.

If we would have followed that philosophy when we worked on Apollo and when we worked on the Shuttle, those programs would not have been successful. They couldn't have been successful. But just because of the little things that you find out, like I was talking about the thermal problem on the lunar module, and there's also kind of an innovative spirit that we had back in those days.

I remember, you know, they were flying the Shuttle piggyback on top of a [Boeing] 747. Most people don't understand how that got started. When we first started developing the Shuttle, we had just let a contract with GE [General Electric Company], I believe, for a huge jet engine. We strapped two big jet engines on that thing and put enough fuel in the payload bay to get it to take off, and then we were going to fly a DC-10 along with it refueling it on the way to the Cape because it would drink so much fuel that you couldn't carry enough fuel to go very far in it.

John [W.] Kiker kept saying, "Put it on top of a C-5A or put it on a 747," and everybody kind of scoffed at that idea. They thought that was the stupidest thing they'd ever heard of. Well, old John Kiker, his wife had cancer at that time, and so he was kind of limited and having a lot of responsibilities at home, yet he sat there at home at night and made him a scale model of the Shuttle and a scale model of a 747, and put engines on the 747.

He and some of his buddies would get out here on these farm-to-market roads on weekends and drag them behind a pickup truck with somebody in the pickup truck playing out cable with a bridle on those things to trim them to where they'd fly. Well, it turned out he couldn't get the Shuttle to fly using the drawings that was available at that time. He had to move the wings back another foot and a half from where the drawings said they ought to be to get it to fly. So we learned something from that right quick, that we had to change the design and move the wings back on it.

But one day he brought his models out to work and went out on that radar range and invited Chris [Christopher C.] Kraft [Jr.] and Dr. Faget and Bob Thompson, all of the management, invited them to come out and see his demo [demonstration]. He took off, had two controller systems there, and he started the propellers on his 747, those little model airplane engines on that thing, started them, he taxied off down the runway and took off with that thing, and circled it around up there until he got it up pretty high, and then he put that on autopilot and punched the button on there, and the Shuttle just popped off of that thing just as neat. Then he flew the Shuttle back down, landed it, and then brought his 747 back down and landed it.

Once they did that, it was everybody's idea. Everybody wanted credit for coming up with that brilliant idea, but it was the persistence of John Kiker in believing something that caused us to cancel that big contract we had on the jet engines and buy a 747 and fly that thing piggyback on a 747. It also gave us an opportunity to make some landing tests where we'd fly it up, pop it off the 747, and fly it back down to a landing.

BERGEN: In 1979, you were promoted to the position of deputy chief of the Propulsion and Power Division. A year later, you were promoted to chief of that division. What were your responsibilities in those roles?

POHL: Well, when I picked up the Propulsion Power Division, I basically picked up all of the other systems. We had a responsibility for all of the plumbing in the Orbiter for the main engines. You know, Marshall [Space Flight Center, Huntsville, Alabama] had the tanks and Marshall had the engines, and everything in between there Johnson Space Center had.

POHL: So we picked that up, picked up the fuel cells, the supercritical cryogenics that powered the fuel cells, the OMS [Orbital Maneuvering Subsystem]. I already had the pyrotechnics and the RCS system, so I picked up the rest of those systems and supervised—no, you didn't have to supervise those people. They knew what they were doing. All you had to do was kind of keep them coordinated a little bit.

BERGEN: Why don't you tell us about your memories of the first Shuttle launch and mission.

POHL: The first Shuttle launch, that was kind of a departure from anything that we had done before. You know, always before, we would fly unmanned flights and make sure that you worked out all these bugs without too much risk of human beings. The Shuttle, the decision was made to man the very first unit, which I personally thought was a mistake.

BERGEN: Why?

POHL: Well, I didn't see any need in risking humans on the first flight, and, number two, I didn't think humans would be as proficient as automated equipment. By that time we had the know-how, and we could build these robots or the automated equipment that can detect things long before a human can detect it, and I thought the vehicle was going to be so difficult to land that we really ought to land it with automated equipment.

I think the primary reason that we didn't do that is, the astronauts were afraid that if we ever landed the Shuttle in an auto mode, they no longer would have a job. And every astronaut, it's their desire to be able to fly that thing at least once and land it exactly on the center line, exactly at the spot that they had picked out prior to launch to touchdown, and they practice, see how close they can hit that spot.

You know, if we landed at one time auto-land, then we'd probably land it again auto-land, and again, and finally that would be the way of—and we finally did put auto-land capability in that vehicle. I had several of the astronauts tell me that if I wanted to test it, they would love to test it, but they couldn't take that position officially, because there was too many of them in there that was afraid that if we did it one time, then they would be out of a job of practicing landing with those G-3s, I guess, that we got, that we converted to trainers.

So we've never used auto-land on the vehicle, but that's one of the things I felt very strong about, that we ought to do that. Never did trust the ejection system that we had on Shuttle. On the first one, we'd blow the top out of it and shoot the crew out through the holes in there. We only had two people in there the first two flights, but I must say that John [W.] Young and [Robert L.] Crippen were gutsy people to crawl in that thing on the very first flight and take off and fly around the world a few times and then come back and land it.

I think the most frightened we ever got was when they crawled out of that thing, and John Young went over—you know, he was so excited, he was doing his arms this way, and he went up and kicked the tires. Now, I was really worried about that, because those tires have got about 375 psi pressure in them, and I knew the brakes got very, very hot on it, and I knew the tires got very hot, and I was afraid the tires were going to explode on that thing when he went up there and kicked the tires. I tried to get word to him out there to tell him, "Get away from it. Get away from it." But nothing happened. It would have been a shame, you know, to do all that flying and that terrific landing and then have a tire blow up on you because you went over and kicked the tires on it.

BERGEN: So all your systems ran successfully on that first mission?

POHL: Yes. We didn't have any major problems on the first flight. We did have problem off and on with the valves on the RCS leaking a little bit and creating a little bit of a problem. We had to change out a lot of engines for a while until we got a little better valves in there.

In one of the early, early Shuttle landing flights, we got a bunch of hydrozene in there because we had a seal to blow out on a pump, and we had to redesign the seals on it. But as I recall—fuel cells. You know, in Gemini, we didn't fly a single Gemini mission that we had much power when we came on. We would lose just about all of our fuel cells and our power was degraded so bad that we had to be very careful about the use of power on it.

On Apollo, the fuel cells, it took seven Ph.D.s to start them and fourteen to shut it down, they were so complex and temperamental, but on Shuttle those things were like a battery. You could think of the fuel cell as a battery with external reactants in it. They'd make a beautiful welder. I mean, you could have it sitting out there idle, not pulling any power, and hit them with 200 amps, and you'd barely see a wiggle in the voltage. So you could turn high power levels on and off just with the flick of a switch, and we had very, very few problems with the fuel cells. Tremendous progress was made in a few years in that area.

Hydrogen leaks plagued us all the way down through the early days of the program. You know, we had valves and seals and things that would leak, even some porosity in the metal and cracks in the main engine. I remember we had a major leak down there on one of the flights where we would—it didn't leak with all of the checks that we could run, but when you start the engine, you got a real high concentration of hydrogen in the bowtail on that thing.

I remember my son and I calculating the leak rate one day going into his grandparents' in Illinois. We sat in the back seat of the car and ran out the calculations on that. And I got them to run a bunch of Tygon [phonetic] tubing all around in the bowtail back there, units here, here, just everywheres in there, and my intent was to see where the hydrogen was coming from by detecting which sensor would increase first. Got the data. It came in. It was all tabular form. I can't make anything out of tabular data. So I started plotting it. Well, I decided—I was division chief then and I decided, shoot, I got all these young kids out here, I'll give it to them. I walked in there, and the first one I came to, I give him this data, and I said, "I want you to plot it," and then I went back in the office, you know, and I twisted my hands. I didn't have anything to do, fidgeting around.

I went back in there to see what kind of progress he was making, and he wasn't doing anything, and I got really upset with him, and I said, "I need it. I need it bad."

He says, "Well, I can't get on a Vax [phonetic] right now." You know, that's when we had these big computers, and we just had terminals in the office, and when too many people get on, you couldn't log on, and it would run very, very slow.

I says, "You don't need to do that. Plot it by hand."

He says, "I don't know how to do that." [Laughter] He says, "It's almost quitting time, and at 4:30 I'll be able to get on. You're not going to do anything with it tonight anyway. I'll have it for you in the morning when you come in."

Well, the next morning when I came in, he had it all plotted out nice and neat, and the computer really makes it look pretty, but there wasn't a single chart in there that had the same scale. You know, the computer auto-arranged all of the scales on it. Well, they didn't do me any good that way.

So I went back in there and says, "Hey, I've got to have it all on the same scale."

He says, "Well, I don't know how to do that because the computer automatically arranges it." But he did. He figured out how to do that, and we got it all.

Then I called down there to the Cape after I looked at the data and told them where to look for the leak. When the technicians was crawling back in that area, one of them heard it hissing, heard the helium hissing, and put his hand around there, and that's when we found a crack about an inch and a half long in the high-pressure manifold on the main engine. It turned out it was a stress corrosion crack because they had done some rework on it, and that copper plating that they had put on the inside had burned away where they did the rework, and so we had a stress corrosion crack in there we had to—and if we wouldn't have found that, you know, that could have been a catastrophe. But we've been very, very lucky and very fortunate that most of those kinds of things have been detected before they created major problems.

BERGEN: How did the responsibilities of your division change after you had a functioning, operational Space Shuttle?

POHL: Well, that division, you know, up until *Challenger* [STS-51L], the responsibility shifted more from an R&D, research and development, activity to a production activity. We still had people watching everything that was done on all of those subsystems. We had subsystem managers, or what we called subsystem managers, and we had improvement programs going on in a lot of the areas where we were having problems with APUs and the valves, and the water boiler, I guess, on the RCS valves on the main engine plumbing looking for better seals and different seals to cut out on the problems we had with the hydrogen leakage in the vehicle. So up until *Challenger*, then, it remained pretty much the same.

After *Challenger*, then I moved up to director of engineering, and then I picked up responsibility for all of the engineering and all of the disciplines out there.

BERGEN: In 1986, the *Challenger* accident occurred. Looking back at that time, especially having experienced Apollo 1, what impact did Challenger have on NASA and JSC specifically, from your perspective?

POHL: It had a real big impact on the way that people thought at the Center. The night before *Challenger* blew up, if you would have walked out there and asked any of the 7,000 people or so that we had on site there at that time, contractors and civil service people, 98 percent of those people would have told you that nothing could happen to that vehicle, we had so much redundancy in it, that nothing would happen to it.

Now, there was a lot of us that lived in areas like the propulsion area that was well aware of the risk involved and the stresses imposed on all of that stuff. You just look at one of those main engines when they start up and you watch those bells all fold up like a piece of paper blowing in the wind, you wonder how they withstand that stress time after time after time.

I really thought if we had a problem we would have a problem with the main engines and not the solid rocket motors, and we really should not have had that problem with the solid rocket motors. That was one of those cases where people didn't understand the mechanism of that joint and how the joint reacted, and they were working on the wrong problem. You know, we blamed that off on cold weather, yet I am convinced, and I will always be convinced, that it could have been a hot day and that would have happened and it would have most likely happened on that flight because they had a lot of difficulty getting that joint together. They most likely shaved off one of those seals or maybe both of them when they put it together.

When they ran a leak test on it, the leak test was not a valid test because they put enough grease in that joint that when they put it together, they filled that grove up in between the seals with grease and then you had a major flaw in the seal. Two feet from that test port and you would have never detected it because it would not have pushed the grease out in the ten minutes that they run the test on it.

If they would have ran the right stress model on that joint, they would have discovered that that joint actually opened up instead of closing up when you put pressure in the motor. You know, within a day after the thing blew up, here at the Johnson Space Center the test people had already modeled that and discovered that that joint was opening up, and we came up with a quick fix. We could have put a belly band, a carbon carbon belly band, about six inches up from those joints and blow those joints, they would have actually made the joints close up when you started the motor instead of opening up, and we could have kept on flying. We didn't have to go back and redesign that joint when we did and delay all of the flights for that period of time. And Max wanted to do that, but he was never able to sell that early on.

BERGEN: Why do you think that was?

POHL: Well, it was such a shock to everybody that people got in the mode of not wanting to take any more risk at all. We even looked at flying that Shuttle, a few flights with automatic controls and flying it unmanned, but we ran into a lot of resistance with the Astronaut Office on that, and that wasn't successful, and I think that people that developed that motor, that was managing the development of that motor, didn't want an easy fix, a quick fix, because it would look like we ought to have fixed it a long time ago because it was too easy to fix.

I had some people from the oil patch come down here and talk to me right after *Challenger*, and it was five of them, and the PAO [Public Affairs Office] called and told me they were there and wanted to talk to somebody in propulsion, and I told him, "Send them over. I'll talk to them."

And the guy told me, says, "I haven't been able to sleep for several nights now." He says, "NASA developed NASTRAND, and that's one of the finest stress programs that was ever invented, and we use it everywheres, all up and down that ship channel down there, and we have bad, bad things down there in the ship channel." He says, "If they get loose, some of those gimbals get loose," he says, "more than five or six people are going to get hurt, and hurt bad." And he says, "PAO gave me the drawings to your solid rocket motor, and we'd modified NASTRAND, and we'd made it what we think is better for our applications, and I put it on my computer. According to my analysis, when you do your pressure test on the case, it yields, and it ought to be bigger in diameter after you run the pressure test than it was before."

And I said, "It does."

He popped his fingers, turned to the other guy, and said, "I told you. I told you." And he reaches down in his briefcase and pulls out these beautiful, colored charts and just a slice through that motor, and they got most of it green and blue, but right down in this one area here it starts turning orange and then yellow, or orange and then red. And he says, "Right there it's yielding. Right there. That's the stress concentration rate there."

I looked at that, and I said, "What in the world did you do that on?"

He says, "On my Cray [computer]."

I said, "You got a Cray?"

He said, "We've got seven of them."

And JSC didn't even have one of those machines then to do that kind of work on, but when I moved up as director of engineering, I made sure we got one. I thought if those people could have seven of them, we could have at least one.

BERGEN: When Aaron Cohen asked you to be director of engineering to get the Shuttle back to flight status, how did you feel about taking on that responsibility at that time?

POHL: Well, Aaron called me over there right after the accident, or pretty soon after the accident, and asked me to take over the engineering directorate, and I told Aaron I couldn't do it. My wife had cancer at that time, and she wasn't doing good, and I just didn't think I could do justice to it. I didn't think I had the time or the energy to do all the things that I needed to do. Matter of fact, I even went back over to the office and wrote down names of three people that I thought would do a good job, and took them back over to Dr. Cohen to suggest that he select one of these people, that they were all people I had a great deal of confidence in.

Well, he didn't do it, and he didn't fill that job. Max Engert [phonetic] was the deputy in there, and poor old Max was run ragged in there trying to do that work, and I really, really felt bad about not doing it and he just leaving it vacant, and it stayed vacant for months. Finally he called me back over there and asked me to reconsider, and I told him—I didn't have the heart to tell him that time that I wouldn't do it. I told him I'd do the best I could on it, and so I did, I took it over.

It was not nearly as much fun as being division chief, and being division chief was not nearly as much fun as being a branch chief, and being a branch chief was not nearly as much fun as being an engineer, but, you know, the higher up you got in the organization, you had to worry about different kinds of things. I love to build things, design things, test things, I love that kind of life, and when you're up there, you don't get to test your theories. You can let somebody else test theirs, but you can't test yours.

BERGEN: What kind of game plan did you have when you took over that position to accomplish that goal?

POHL: To operate one day at a time. [Laughter] I guess the first thing I did was I put in a system, an inventory system, so that I could understand how much equipment each division had, when it was purchased, the class life of that equipment, so that I could budget for replacement of that stuff. You know, some of them, they're always trying to snooker you into something, division chiefs would, and they always had to have these new machines, these new computers, and I didn't know what they had. So that's the first thing I put in, was a system where I could take the NIMS database and by adding a few things and changing that around, we could plan five years down the road on what equipment we needed to replace.

I guess the second thing I did was I took some of the monies that engineering had and gave them over to the tech services, to the shop, so that the shop could buy some modern equipment. All of the equipment in the shop at that time was basically World War II equipment, you know, that came out of the ships and things after the Second World War, and we bought it real cheap and put it in there. And with good machinists, you know, they can do first-class work with that kind of equipment, but there was new things on the market. You know, we got an EDM [Electric Discharge Machine] machine, and then we got a water knife, and I gave them a little money because the feeling I had was that an engineer is worthless if they can't design something, and if you design it, then you have to build it, and if you're going to build it, you need to use the kind of equipment that industry will be using if you go out and make it for some project like that. So we did. We put in a pretty good machine shop over there.

BERGEN: What was the biggest obstacle that you had to overcome in your new position?

POHL: Budgets. That was always the biggest problem, was working the budgets and trying to keep the budget balanced and trying to get the money to flow in those areas where you kind of kept things on an even keel. You know, lots of money would go into computers and software and those kind of things, and mechanical systems like structures, mechanics, propulsion and power, those people generally come out on the short end of the budget cycle, and you've got to plan the budget two years, three years down the road. Really three years. I learned that pretty quick, because nobody pays any attention to what you put in a budget two years down the road. They're only worried about this year. So if you want to get something in, you don't put it in here, you put it in two years down the road.

Well, the two years come by pretty quick, and then when they want to take it out, you say, "Well, you let it go the last two years. Now what has changed?" And they feel guilty then because they didn't look that far down the road and didn't see it coming into being. So we were able to get a few things going in engineering that other people hadn't paid a great deal of attention to.

BERGEN: When STS-26, the return-to-to flight mission, came up, were you confident about its success at that time?

POHL: Yes. Absolutely. Like I said, I would have been confident to launch it within two months after *Challenger*, because I thought we had that problem under control, and I, really, to tell you the truth, was more worried, more concerned about the long delay between the flights than anything, and primarily the technicians and the people that put those things together check them out. You know, if you're doing something every day and it's kind of routine, you kind of remember what to do, but now, if I don't do it for nine months or a year and then you try to start up, and you try to think of everything, even though it's all written down on paper, you know, you don't exactly remember how you went about doing those things. I was most concerned about the first launch, that they would miss something.

I think probably one of the major, major contributions that I made to the Shuttle Program happened after [STS-]27. On both 26 and 27, we had major problems at the Cape because our winds were out, and at that time we used four seasons, four winds. We had a summer wind, a fall wind, a winter wind, and a spring wind, and that was a standard wind. They had sent balloons up for ten years down at the Cape, and they had measured the velocity and the direction of the winds, and they'd put all this data in these computers, and this computer came out with a mean wind, an average wind for those seasons, and that's what we loaded in the computers.

Well, there is no such thing as an average wind. On [STS-]26, we couldn't launch because the wind was too low. Now, how do you explain to the media people that you can't launch because the wind's too low, you've got to wait and let it pick up a little bit? On [STS-]27, our indicators were out 102, 103 percent on several indicators on the winds, and I know I got on the hot phone, on the red phone, and told Truly to launch it, it was going to come in at about 82 percent. And what I had done, between 26 and 27 I made it my business to understand how they calculated the indicators. What they were doing, they'd send up a balloon four hours, eight hours, twelve hours before launch, and they would measure the wind, and then they added 50 percent on to that wind because the wind could change that much from the last balloon until you launched the vehicle. I mean, when they collected all this data for ten years, there was data that indicated that the wind could change this much in four hours. Well, yes, if a jet stream moves in or a northern moves through, when that hits, the wind's going to change, especially if it's a strong northern or the jet stream swings over in that period of time, you know, if it's real close.

Well, we had been watching those winds, and they were decreasing a little bit, hadn't increased in the past twenty-four hours just a little bit. There was no jet stream close by. There was no cold front close by. So there was no reason to add that 50 percent on there. I explained all of that to Truly, and within the last seconds he gave the go-ahead to launch it. I was on cloud nine when that thing got in orbit. You know, if it would have blown up for anything and I told them to launch it, which was a departure, but I really didn't feel comfortable with deservicing or unloading all that hydrogen. It's very, very dangerous to service and deservice that thing, and I thought it was more risky to cancel the flight and offload all of the propellants than it was to go ahead and launch it, and I was confident it was going to come in at about 82, 83 percent. Well, it didn't; it came in at 86, I believe. So I missed it just a little bit, but not that much.

Well, that set off a major investigation. I know Chris Kraft and Max Faget and somebody else was on that committee to look at it, and that's when I came up with DoLILU [Day of Launch I-Load Update]. I don't know whether you've heard of DoLILU or not, but I explained to them how we could program those computers to the actual winds that we had measured four hours before liftoff and not have to go with this mean average wind each time. And it was not easy to sell that. People worried about it. They were worried about getting the wrong code in or making an error, and they wanted everything tested, so we kept our sail up out here and kept it a long time.

But we went to DoLILU and then DoLILU 2, and right now there's going to be very, very few times when you ever scrub a launch because the wind is too high or too low or out of limits. You can program that vehicle where it'll fly through almost any wind; 99.9 percent of the winds, you can program it to where it will fly through them. You'll have to worry about lightning and clouds and those kind of things, but you won't have to worry about wind. I really feel good about that now.

Chris never did like that. John [F.] Yardley was on that committee. Man, John picked that up right quick. He loved that idea of DoLILU, and Max picked it up real quick, but Chris was very conservative. He was very, very much afraid that we were going to screw up the software someplace and have a problem during a launch, but it's worked out very well, and probably improved the safety of the vehicle more than any single thing that we have done because it doesn't stress the vehicle as high now as it used to, because every time you launched it before, you were running close to the limits, structural limits, of the vehicle.

BERGEN: Is there anything else from your work on the Shuttle that stands out in your mind?

POHL: I guess the other thing that we made a lot of progress on was the tile and the way we put the tile on and the way we got them to stay on, came up with the densified tile, where we put a very dense layer right at the surface so that when you glued it on, it wouldn't peel off at the glue joint. They lose very few tiles now. On the first launch, that was one of our major worries, was we lost a bunch of tile during launch, and we didn't know whether it would burn up or not when it came back in.

That's finding its way into a whole host of other industries. They're using that tile now in a lot of the furnaces, a lot of the powerplants, and things like that. It's cheap, it's reliable. You know, the interesting thing about that tile, you can take a one-inch by one-inch tube of it and you can heat it with an acetylene torch until it's white hot and reach down to pick it up on the corners and it won't burn you. You've got enough moisture in your finger where it'll cool it down enough to where it won't burn you. It has so little thermal content, so little heat content in there, and it's very, very light.

BERGEN: I've held a piece before, and it's amazing how light it is.

POHL: Yes, and how good it works. Of course, you can sloppydiddle [phonetic]. It'll melt if you get it too hot, but you've got to get it pretty hot to do that.

BERGEN: While you were director of engineering, what types of activities did your directorate work on aside from Shuttle?

POHL: Well, we always had an Advanced Programs Office that was working on new concepts, new vehicles, new designs. We designed an awful lot of experiments that we flew in the Shuttle, and those arms that we used to lift the payloads out, some of those were designed in-house and put on Shuttle.

We worked on spacesuits a whole lot and trying to get the spacesuits improved. You know, most people don't realize it, but when you pressurize one of those suits and you get in a vacuum, it takes about 80 percent of the astronauts' energy just to move their hand up. You know, they just want to go out straight, and to reach his chest controllers, they really have to strain to get to those, and looking at joints that would not be sensitive to pressure.

We got into farming a little bit, growing wheat and things in the chambers over there, looking at long missions in space like on Space Station where you could grow some of your food and use the plants to make oxygen so that you didn't have to carry—to eat up the carbon dioxide and give off oxygen. And that's proven to be fairly successful, I think. Matter of fact, I suspect we're going to have some of that stuff on Space Station.

Of course, we're working on Space Station. That was a shame, the way that program went. You know, it looked like every administration that came in wanted to redesign it. That's when I was telling somebody about all of these congressional aides we've got up there. You know, at the time I left up there, they had about 20,000 of those people writing the laws, and they were all bright young lawyers from these prestigious Eastern law schools, and every one of them was absolutely convinced that with the right jury they could change the laws of physics. And they all wanted to do this and wanted to do that, and they each one had their idea as to what they wanted to do, with no real concept as to what it took in terms of dollars and time to make some of those things happen, and sometimes it's just not feasible to make it happen because it's counter to the laws of physics. But that was one of the major things that I found after—I guess really started after *Challenger*, is that we got a lot more interference from Congress in our daily activities, our design activities.

Back in the Apollo Program, when they first put this Center in, you know, all Congress and the White House was concerned about was where the site was going to be. It was no accident that the Manned Space Craft Center was put in Houston, Texas, and the facilities put in New Mexico, and the facilities put in Louisiana. That's where the center of power existed out there. But they didn't worry at all about the engineering, the design, any of that kind of stuff. When Shuttle came along, the facilities were all in place so they couldn't worry about facilities. They worried about where the contracts was going to be let, you know, what districts or states was going to get contracts, and that's kind of the way it was spread around.

When Space Station came around, there was no really big contracts. There was a lot of little piecemeal, but they tried to break it up so that each Center would have a certain—you know, Marshall would have a part, JSC would have a part, Lewis [Research Center, Ohio—now Glenn Research Center] would have a part. They were going to take the money and spread it out to as many Centers as they could. That created a huge interface kind of a problem, and then you got multiple contractors involved managed by different Centers. And Congress got in there, and each one was pulling for their state, the Center that was in their state, and trying to shuffle things around, each one vying to try to get a little bigger slice of the pie.

There was a difference in the attitude of the people who worked on Space Station, too, across the board in the early days. You know, back on Apollo, I am convinced that one reason that program was so successful was that 80 percent of the people that worked on it wanted to beat the Russians to the Moon. That was their main drive, was to beat the Russians to the Moon. It was kind of a game between the engineers as to who was best and who could be first. We worked a lot of hours, long and hard, with that objective in mind of getting there before the Russians got there.

But then, after Apollo, we had that huge turndown in manpower and laid off lots and lots of people. When Shuttle came about, the feeling was that if we didn't build it within the time, within the cost, there would be no NASA, nobody would have a job, Johnson Space Center would be given back to Rice University, and they would move the campus out here and have a beautiful campus. So everybody worked on it with the thought process if we didn't do it quick and we didn't do it cheap, there would be no agency, and that was the drive.

Then when Space Station came on, it was kind of a job, and we had promoted people into high-paying management jobs. That's where you take a good engineer and you ruin them by making a manager out of them and giving them an increase in salary, and they made lousy managers because they were good engineers. Well, we got all of these managers in that had a little authority, and they were more interested in throwing their weight around than they were in trying to figure out what was the right thing to do. You know, if it's a Marshall guy, they were going to protect Marshall's interest. If it was a JSC guy, they were going to protect JSC's interest, rather than looking at and trying to figure out what was the best for the program and what was the right thing to do, "I've got the authority to tell you to do it, and I'm going to tell you to do it, regardless of whether it's right or not," and I think that attitude really hurt the Space Station Program in the early years. Still, we wound up with a station that is way too difficult, way too complex, simply because we were trying to satisfy too many different requirements.

BERGEN: Seems like a big change came after President Clinton called to change the Space Station in 1993. What were your thoughts at that time?

POHL: Well, that was one of the many changes that we went through. You know, that was probably about the third or fourth one since Space Station had been started, major changes, and it came at a very, very unfortunate time, because we were faced with many complexities, many difficulties, and we had just let things freeze long enough so that things were beginning to go together. We were producing hardware, we were cutting chips, and we were building Space Station when they came in.

I guess the major policy shift there was that they wanted to bring the Russians in as an equal partner. The contractors were fighting between each other, Centers were fighting between each other, so they kind of took the rest of the Station Program and lumped it together where Marshall would do the engineering and JSC would do the operations on it, and bringing the Russians in meant that you had to redesign the whole thing because the Russians were going to provide this module and this module, and we were going to provide one here and one here, and somebody else was going to provide another one over there. So we basically scrapped everything again and started over with what looked like the same station, but many, many changes were made, and the configuration that we're making right now is a very, very expensive configuration right now.

You know, I handled that Option C for JSC when we came out with those three options, and if they would have went with Option C, we would have a Space Station up there now, we would have had it up there a couple of years ago, it would have been cheap, it would have helped the infrastructure of the Shuttle Program a whole lot because we were using all Shuttle components and Shuttle computers, Shuttle MDMs [multiplexer/demultiplexer], a life-support system that we had in Shuttle. We had solar arrays on it, but we took all of those—switch gear out and hooked it up like you hook up a car battery and alternator, you know, the engine starts the alternator, charges the battery, and then you shut it off and you drain out of the battery. So as it went through the day/night cycles it would just automatically change over, very simple, very cheap, didn't provide everything that everybody wanted because it had kind of a reversing microgravity in it because we were flying it around this way, we had the solar arrays fixed, and they didn't rotate, to make them cheap and simple.

We used all of the tooling for the external tank to make that container. We had a huge container, a big volume [unclear]. The problem with it was the Europeans didn't like it because it didn't have anything in there for them. The Japanese didn't like it because they wanted their module up there, and even though I designed places for them to stick those modules, external on there, sticking out, they knew that we didn't need that because we had more than ample volume in there, we didn't need that volume, and there was nothing in it for the Russians to do.

This administration was absolutely convinced that we had to bring the Russians in as an equal partner, otherwise they would peddle their technology to some other Third World country or something. This way we could keep them in our fold. And who's to say that's not good logic? I don't know. As far as getting a space station up there, it wasn't good, but from an overall standpoint, I guess it was okay.

BERGEN: What do you think, if you could point to one thing [that] finally doomed Space Station Freedom?

POHL: Well, I think the thing that really killed Space Station was when Gene Kranz went to Aaron and told him he had to have this huge increase in money to build the Control Center over there for it, and he did that in front of all of the contractors, who then went home and said, "We'd better get our acts in the water, too, because Gene's going to get all of our money. There's only so much money, and if you give Gene more money to build his Control Center, then somebody else is going to get a little bit less."

So McDonnell [McDonnell Douglas Corporation] turned right around and came back within a week and said they had to have a big increase in money. Well, that went right up to Washington [DC] right when the administration changed, and I think that was the impetus that caused them to cancel Freedom and start looking at other options, that and the current administration where they put their Good Housekeeping stamp of approval on the design.

But I think had Gene not gone to Aaron and—Aaron was in Washington at that time, but he was coming back down here every other Friday, I believe, and getting a briefing on it, and I think had Gene not gone in for this huge increase, which prompted, then, McDonnell to come back to get their oar in the water to get their fair share of it, I think Freedom would have went on and would have been built, and I think it would have been a good station, probably at half the cost of what we're spending now. But once those cost increases made it up into headquarters and right on into Congress, then everything blew up, and I'm convinced that's what killed Freedom.

And truth of the matter is, we didn't need those Control Centers. We didn't need that much money in there for those Control Centers at that time. You know, schedule was slipping a little bit. All of that could have slipped down a year or two downstream, and it would have still been fine. But Gene just came in with an ultimatum, you know, "If you don't give me this money, I'm not going to have a Control Center. We're not going to launch it."

BERGEN: It's been a long struggle for Space Station.

POHL: Yes, it has, and, you know, I feel so sorry for those people that are struggling with that thing and working on it. You know, we've got some very, very good people out there that's spending very, very long hours trying to put it together and to try to make it work under very, very difficult circumstances. You know, just the difference in culture between the Russians and us and between the Japanese and us, you have to kind of understand their culture, you have to kind of understand their background, their experience levels, and they have to kind of understand ours, because we just do things different.

The Russians do things more like the French. As a matter of fact, there's a great deal of similarity between the French engineering and the Russian engineering. They think a whole lot alike.

BERGEN: Did you have much interaction with the international partners?

POHL: A fair amount. I really didn't have any interaction with the Russians. My interaction was primarily with the Europeans and with the Japanese. I remember very clearly going up to Washington to make a presentation on Option C. Option A went and made a briefing, and then Option B, and then it was my turn, and I got up there. The guy in charge of the Japanese module asked me how I was going to put his module up there. Well, you had the same problem with Option A and Option B, because the module weighed too much, and we didn't have a vehicle in this country that could put it up. And I never hesitated, I just told him we're going to put it up on a Russian booster. And we had looked at it and decided that, yes, we had plenty of capability to do it with this Russian booster, so I just made the comment, "We're going to put it up with a Russian booster." He went bolt upright, stood at attention, and says, "Not acceptable. We're still at war status with those people," and sat back down.

Well, that was the first time I realized that they hadn't settled some of their differences on some of the borders over there yet someplace. But that was some of the problems we ran into.

BERGEN: Adds a whole new complexity when you have to work with other countries to accomplish an engineering task.

POHL: Yes. Yes. And they have different tools, they have different machines, their analysis programs are different, everything is different, and you have to kind of—if you can understand what they're doing, you have to understand their tools and how they go about doing things and how they stress things.

BERGEN: After the redesign phase was over, you took a new position, which was of chief engineer of the International Space Station.

POHL: No.

BERGEN: No?

POHL: No. What I did was, I agreed to go over and manage Option C, the design. You know, we had those three designs. Langley had one, Marshall had one, and JSC had one, and I think Langley's was Option A and Marshall's was Option B, and I had Option C.

Option C basically was taking the external tank and converting it into a container, pressure container, for you to put all of the equipment in. We would launch it in place of the

Orbiter. You hang it on the side of the external tank and launch it, one launch, put up their complete space station, and then the next launch is, you'd go up and you'd be servicing it. It was a ninety-day activity, and I headed that up for the Johnson Space Center. When that was over, when that was finished, then I retired. I guess I hung around a little while, because I stayed until Aaron left, and then when Aaron left, I left.

BERGEN: You seemed to work a great deal with Aaron Cohen. Why don't you tell us a little bit about him.

POHL: Yes. We had a good working relationship. I thought the world of Aaron and tried to help him and tried to do what I thought was right. We had a lot of fights, we had a lot of arguments. I remember Hank Flagg [phonetic] coming down, he was chief legal guy, and told me that I made Aaron mad one day and I'd better be looking for another job; you don't tell the boss those kind of things. And I told Hank, I says, "Oh, that ain't the first one we've had and it won't be the last one."

You know, I always felt like it was my responsibility, even though it really pains me and I hate to give somebody bad news, but the biggest problem management has is not getting correct information, and people wanting to look good and telling the boss something that's not necessarily right just because it sounds good or looks good. And I would never do that. I would always tell Aaron what I thought, and I always operated on the philosophy that my first responsibility was to tell somebody what I believed and do my very best to convince them I was right, and then if I couldn't convince them I was right, then I had two choices. Number one, I could quit, or, number two, I could do it their way. That's kind of the way that I operated.

Now, Aaron had a—you know, he had a lot of problems. Aaron is the first guy that ever took on a project and managed that project from inception to flight. You know, he was the Orbiter project manager. He's one of the most tenacious guys I ever saw in my life, you know. He never gave up, and he gave it 150 percent every single day. Five, six, seven days a week, he was in there. I helped him a whole lot. Like I say, we had dual tandem actuators and four APUs, and the vehicle was way too heavy, so we cut that down to single actuators and three APUs, and that was kind of a tough sell.

We had great big doors on the front of the vehicle for the RCS that would open up in flight and then close up because people didn't think the engines would withstand the reentry temperature. Well, we ran enough thermal models here in-house, and Norm [Norman] Chaffee was really good at that, to convince ourselves that we didn't have to close those nozzles up, that they would withstand the entry heating. So we went and presented that to Aaron and was able to sell it, because it saved about two or three thousand pounds of vehicle weight, and actually would have been more than that because they didn't have enough weight in the design at the head, and when they started detailing out, the weight was going to go up. And we changed from monopropellant to bipropellants, a lot of things like that that was very, very hard sell.

They were going to put Centaur in the Shuttle, and it got overweight, primarily because the GSE [Ground Support Equipment] that they put in the vehicle was so complex and so heavy because they wanted to put all of these safety things in there. I went out to GD [General Dynamics Corporation] one time, on a trip out there, I spent a couple of days out there with them, and we totally redesigned that whole system, threw out all the redundant valves, the redundancy in that thing, and made it very simple, very straightforward. And I am absolutely convinced that we enhanced the safety of it rather than degrading the safety of it, but yet that was a tough sell, to come back here and sell it through the system. The liability people, the safety people, people that worked in management that never designed or built anything that looks at schematics and things like that that wasn't convinced that we did the right thing probably was as much upset over the fact that I went out there and made a bunch of decisions and did a bunch of things without their concurrence on it, but, anyhow, we did that.

But Aaron and I worked together a long time. I knew Aaron back in the Apollo Program. I didn't work real close with him back then, but I knew him. And then after he took over as Orbiter project manager, we worked very, very close together and spent a lot of nights out there arguing about things where Aaron would be on one side of the fence and I'd be on the other side of the fence and we'd go round and round the mulberry bush, and he'd try to convince me that I was wrong, and I'd try to convince him that he was wrong.

I can remember one night staying out there until about 8:30 at night, he and I just going around and around the mulberry bush on something, and next morning at 7:00 o'clock he calls up and wants to know if we're still friends. "Yeah, why not?" I mean, just because we don't see eye to eye on something is no sign we can't be friends, and we ought to be able to argue about things a little bit so that we get it all out on the table and each one knows where the other one's coming from.

But he was a great guy to work for, I thought. The main reason Aaron was such a great guy to work for is that he never did have a hidden agenda. You know, he called it the way he saw it, and he was always honest with you. And what more can you ask in a guy? You know, the biggest problem you get a lot of times with managers is they've got some kind of hidden agenda, you don't quite understand where they're coming from or what they're doing.

Joe [Joseph F.] Shea was another guy that I enjoyed working with, and a lot of people couldn't get along with Joe. Joe had a big ego and had a big chip on his shoulder, and you had to handle Joe in a particular kind of a way, but he was a very, very sharp engineer, and if you presented things in a way that a reasonable engineer would come to this conclusion, you could just—if you went in and told Joe what he had to do, he'd find ten thousand reasons why he didn't have to do that, and it would not get done. But if you go in there and tell Joe, "Here are the facts. Here's the situation," and you put the facts together in a certain way that a reasonable person would draw this conclusion, he would jump to that conclusion every time and say, "Why don't you do this?"

Then you could say, "Joe, I think that's a good idea. I think we ought to go do it." And he would buy it. But too many people would go in there and try to tell him what he had to do, and it just would backfire with them.

Kenny [Kenneth S.] Kleinknecht was another interesting guy to work for. Now, Kenny, about half the time would make the wrong decision the first time, and if you argued with him about it, you'd just reinforce it. You know, he'd just go hard over. But if you'd just stop and wait until the next day, call him up and say, "Kenny, you know we made this decision yesterday. I've been thinking about it. I think we made the wrong decision," and tell him why, and he'd clip over and make the other one. You know, too many managers, when they make a decision, they didn't want to admit that maybe they made the wrong decision, and they wouldn't change until forced to change someplace. But Kenny had no problem with changing his mind. If he made a decision and made a wrong decision, he'd change it the next day just as quick and just as easy.

I remember taking a memo over to Kenny one time to sign, to go out, and I had a whole bunch of carbon copies on it, and that was back during the days when you'd put all these carbons in the typewriter and the secretary had to type it. If you made a mistake, you had to go through and erase out all of them, and being for his signature, it had—well, first thing he did was he flipped over to the back and started scratching out all of the carbons I had on there, the names of all the people it was going to go to. I says, "Kenny, don't do that." I said, "It won't hurt for them to get copies."

He says, "Henry, that's where you're wrong." He says, "These people don't need copies of it."

I said, "I'm going to have to go back and retype it."

He said, "Yes, you will retype it," and he just scratched all those names off.

I said, "It won't hurt them to get a copy of it."

He said, "That's where you're wrong." He says, "You send them a copy of it, and they're going to read it, and that's wasting their time to read it." He said, "Then they're going to waste some more time because they're going to think about it and wonder if they ought to do something about it." And he said, "Worse yet, they might do something about it." [Laughter] So he did, he took all the carbons off of it.

You know, we don't think of those kind of things now when you've got e-mail and you put a copy out to everybody under the sun, and you've got a list of e-mail messages that long. Five percent or 1 percent may be needed, and all the rest of them is just wasting your time.

The other problem we got along those lines now is all of the young engineers are very proficient at typing, and things that you could say in three sentences, they'll take three pages to write it all down because most people value their work in the number of pages instead of the content that you put in. Not much thought goes into briefings anymore. I mean, you get on the typewriter and you just start typing, and you make these viewgraphs and take them and put them on the viewgraph machine without really thinking too much about what message you wanted to get across and how you get it across. When you had to use these flip charts, you thought a whole lot about what you were going to put down on that chart, because when you got that Magic Marker and put it on there, you didn't want to write too much, it was going to take a long time to write it down, and you'd think about what you was going to say and how you'd say it to convey the message that you wanted to convey. We don't think a lot about that now.

In engineering school, when I went to engineering school, you know, we were taught that engineering reports should be brief, they should be to the point, and they should make sure that you supported—whatever data you put in there was supported by the words that you put in there. Now, you know, I don't think even in engineering schools they think too much about what they're saying. If it's an inch-thick document, it's got to be better than one that's three pages.

You know, most people don't even understand work anymore. Work is defined as force times distance, and, you know, you can work very hard and not accomplish anything. When you went home at night, if you haven't accomplished anything, you might as well not work. That's one of the things we did, getting back to my early days and early training back in the ABMA [Army Ballistic Missiles Agency], the one thing I had to do every single day, the last fifteen minutes before I left the office, was write down on a piece of paper what I did that day and turn it in. We had an activity report that we had to write every single day. And I thought that was so absolutely stupid to have to take time to write those activity reports, but one of the things I learned pretty quick was there were some days where there was not much activity, and it made you acutely aware of what you did that day and what you accomplished that day when you had to sit down and put it down on a piece of paper and turn it in.

BERGEN: You have had such a long career that spanned over so much progress in space. Looking back, what do you feel is your greatest accomplishment?

POHL: Well, I might have to think about that one a while. You know, there's a lot of little things, but I don't know that I did anything that was really major. I made those little models and put those in the wind tunnels. That was really the most fun job I ever had, doing that. I didn't think it was fun at the time, but it was fun. You know, you could see some progress. You could see the fire and smoke, and you knew that you conceived that, you designed that, you made it all happen, and you did it yourself. You didn't have a committee of forty engineers trying to design it. You did it all yourself, did all the testing yourself. So that was the most fun job that I ever had.

I think, you know, some of the contributions I've made we've gone over already. Taking all the weight out of the Shuttle was not an easy thing, and I brought that forward on my own initiative and fought it through the system, which was not an easy fight. DoLILU, day-of-launch I-loads, I fought that through the system. That was not easy, although I got the support of John Yardley real quick on that one, and that was a big help. A lot of people had a lot of confidence in John.

I think that on Apollo, had I not had the experience that I had with those little rockets and testing them in vacuum chambers and things like that, that we might not have been as successful on the Apollo flights. Those things fired about 140, 150,000 times on the trip to the Moon and back, and we had a lot of problems in the development of those things. I even put a little precup in there to smooth out the ignition a little bit so we didn't break so many chambers on the start-up.

But I would have to go back home and think about what I would think would be my major accomplishment.

BERGEN: Do you have any especially fond memories that stand out in your mind from your career?

POHL: I don't know about fond memories. I have a lot of memories of problems. I remember fighting the battle with the corrosion in the propellant tanks on Apollo. You know, we had three tanks that we put into a qual [qualification] program, and they were supposed to go thirty days in the qual program. Twenty-eight days into the thirty-day program, one of them developed a tiny, tiny little leak. I mean, it was so small you could just barely see that oxidizer coming out through the wall in that hole. It created a little brown spot on the side of the tank.

They cut it out and sent it off to one of those famous testing laboratories and found out that it was a stress corrosion. There are reports that it was caused from a fingerprint in the tank before heat treat, and the salt in the finger caused the stress corrosion in titanium. Well, I got that report in here, and I looked at it, and it looked okay to me, and I signed it off, and I sent Jim Ackerman and Darrell Kendrick out to Bell [Aerosystems Corporation] shortly after that. They came back and told me that we could not let them get away with that, it was not a fingerprint. "Darrell, what makes you think it's not a fingerprint?"

He says, "It would take a monkey to get their finger in that tank. The hole's only that big around, and the tank's that big around, and you had to get your hand through the hole and touch it over on the side over here. It's got a big weld up through here."

I says, "Oh, Darrell," I said, "somebody got ready to put those two halves together, they saw a speck in there, and they reached in there with their finger and wiped it out and then welded it."

Well, he came back in a little while and showed me that the tank had gone through about seven or eight flush fluids. I'm surprised it didn't dissolve the whole tank, much less any fingerprint there was in it. And he wouldn't let me forget that. Every day, every morning, he and Ackerman would come in my office, "Henry, what are we going to do about it? Henry, what are we going to do about it? We can't let them get away with that."

So finally I told them to make me a proposal, tell me what to do, and they came back with the idea of putting ten tanks in test, and if all ten of those tanks went through that test, they would say it was random and write it off as a random failure but not as a fingerprint.

Well, I went out and tried to talk to John Gibb, who was in charge of the program at Rockwell, and he wouldn't do anything. It was going to take contractual direction to Bell, and he didn't want to do that, and he didn't think it was a problem.

So I came back over and talked to Joe Shea about it, and he immediately directed Rockwell to put those ten tanks in test, in thirty-day test. So we did, and less than 100 hours into that thirty-day test, one of those tanks just exploded. It busted wide open, and before they could get the pressure off of the other nine, two more of them blew up. When you looked at the inside of those tanks under a magnifying glass, it looked like caked ground, p_____ ground where water's been standing, it dries up, and it all cakes up, you've got little cracks, thousands of them all over the place everywheres.

We got everybody in the United States involved in that one. We had Langley involved, we had Lewis involved, we had Marshall involved, of course, Rockwell and all the other people around the country involved in that. It turned out that the stress corrosion was caused because we had directed the manufacture of the tetroxide through the Air Force to change the manufacturing process of it, and what they were doing was removing the water out of it so it was absolutely dry, and with it dry, then that left free hydrogen in there, so it caused hydrogen embrittlement in the tank. We had gotten just a little bit of that new propellant on that first three tanks, but the second set we had put in there was all new propellant, and that was the last time we would have exposed those vehicles to that propellant for any length of time prior to the first Apollo flight.

You know, I still think about that and how close I came to letting that get by, and had it not been for the persistence of two young engineers out there that just would not let me forget it and just kept on, day after day after day, bugging me about it, we would let it go, we wouldn't have done anything about it, and we probably would not have found it until the first Shuttle flight—I mean Apollo flight, and it would have probably blown up. Being up there, they would not have had any good data to try to figure out what blew up.

I've thought more about that than any other single thing that happened, simply because it came so close to getting by. I remember John Gibb telling me when I went out there and tried to convince him to put those tanks in test, if I wanted to tell him how to run his program, that I come home and do it through the contracting officer. When my contracting officer directed his contracting officer to direct him that he would do it, but not before, and then he'd tell me, "Henry, don't make an issue out of it. You're a well-respected engineer." He says, "You push this, you're going to lose all credibility. People will never believe anything that you tell them after that."

And, you know, you weigh all those kind of things, but I finally decided we absolutely had to do something on it, only because of the persistence of Kendrick and Ackerman on that. It's little things like that that you come so close to letting go, that you lay in bed at night thinking about what may be other things I've missed or let go.

BERGEN: What was your biggest challenge that you felt you had to overcome during your career?

POHL: Being shy, being an introvert. I still don't like to make speeches. I don't like to get up in front of people. I never did like going to a football game because of the bright lights. It looked like everybody was watching me when I was under those bright lights. I never did like that much. I operated very well by myself. I never had the need to socialize a whole lot or visit a whole lot with the people. Most of the time I was thinking about something or some design or something.

Matter of fact, you know, I used to come up with all these brilliant ideas at night, laying there in bed. As a matter of fact, the time between the time that you wake up and get up is the most precious time in the world, because that's when you can think and your mind is clear and you can think of all of these good things to do. And then I'd get up and go to work and I'd forget them. You think about them in the middle of the night.

So I bought a recorder and set it by the bed, one of these old reel-to-reel tape recorders, and I'd come up with these brilliant ideas. My intent was to turn this thing on and record it so I could remember it the next day. Well, shit, I come up with this brilliant idea, and I get up there and fiddle with this thing, and, "Now, what was I supposed to say? I done forgot." So it never did work out. I found out it was a whole lot easier to just get a pad and pencil. You know all you've got to do is just jot a few words down on it, and then next day you could recall what you was thinking about, and sometimes they didn't seem to be so brilliant the next day as they did the night when you was thinking about them, but other times they did.

BERGEN: You had a long and wonderful career with NASA. What have you been doing since you left NASA?

POHL: Well, I've been doing a little consulting work, not much. Been farming a little bit, or ranching a little bit. I've got a place down in the country north of Victoria [Texas], and I've got a few cows down there. Floods took out all the fences last year, so I've been in the process of trying to put fences back in there. Doing a little consulting work for Kistler Aerospace Corporation, Aerojet. I've done some for Thiokol, and that's been fun. It kind of keeps you thinking about the things that—most people don't realize how difficult it is to make a rocket yet. Ninety percent of the weight of a rocket at liftoff is fuel. That leaves 10 percent for the structure and a payload and electronics and power and everything else.

BERGEN: So what would you like to see in the future of space exploration?

POHL: Well, I guess, you know, one of the main things I think we need to work on is better propulsion systems. We don't put any money in those areas now. You know, the Shuttle engines operate at 97, 98 percent efficient, and they think that's all you can do, and it probably is, but unless you keep working on new techniques and new ways of doing things—I finally saw that they put some money in one of those plug nozzles or aerospike nozzles, which is going in the right direction. At least it's a different concept. There's been theories about that engine for years and years and years, being altitude-compensating, so it runs at optimum performance at sea level and also optimum performance at altitude. So that's going in the right direction. But we've put very, very little money in those kinds of things.

We still put lots of money in computers and software and those kinds of things. Back in the beginning—you know, it's hard to remember that we were still in vacuum tubes when we started the Apollo Program, and to go from vacuum tubes to transistors to integrated circuits, you know, this watch has got many times the computing power that we had on Apollo. In the beginning, we had to develop computers, we had to develop software, we had to force the development of all of those things, but now, you know, that's such a huge industry. If you put the total NASA budget into developing software, into developing computers or communications systems, it wouldn't make any difference. I mean, if you took the total NASA budget, it would be such a teeny little increase in the money that's already spent in those areas, but yet if NASA or the government doesn't develop a rocket engine, nobody else has need for one of those things yet. Nobody else can afford to develop one yet. And, you know, people get old and people die, and if you bring some new guys on, if you don't give them a chance to do something, then you've got to learn everything that you learned all over again.

You know, thermal stresses. Most people don't think about those kind of things. We've got a lot of good computer programs now that will help them design a lot of that stuff, but they've still got to know what to worry about, what's important, what's not important. So I see that as one of the major problems that the agency's faced with, is the distribution of the resources into those areas where, if the government doesn't do it, it won't be done, and decrease the resources going into those areas where there's a huge industry out there now that's going to do it even if the government doesn't put a penny in it.

BERGEN: I found, in doing some research on you, a copy of the engineering directorate patch and a quote from you, actually. I was wondering if you could tell us something about that patch and what that means to you.

POHL: Well, that description of it is pretty much self-explanatory, I guess. What we tried to do in coming up with this patch, as I recall, we tried to come up with something that would cover all of the disciplines of engineering.

Engineers operate on kind of a special kind of a code. You realize that if you make a mistake in a design or have a flaw in something, that people are going to get hurt, and so your designs need to be proven, they need to be on a solid, firm foundation. And that's basically what we were trying to say with this patch, was, come up with something that would have the properties that an engineer could be proud of, the moral characteristics of an engineer. You know, a lawyer can twist the facts up so that they can sway a jury most any kind of way. Engineering is not that forgiving. It's either going to work or it's not going to work, and you need to understand very, very clearly, number one, what your objective is, what you're going to do, and then you need to go about doing it in a way that's going to ensure success.

The basic difference between an engineer and a physicist or a scientist is that an engineering design has to be cost-effective. It has to be profitable, otherwise it's of no use to anybody. A physicist or a scientist, you know, their theories don't have to be profitable, they don't have to be cost-effective. They're reaching out into new territory and new kinds of things, and if one out of a thousand or one out of two thousand bear fruit, then they've done a good job.

I also think a little bit in terms of the difference between an engineer and a pure science major like a chemist or a physicist. Engineers nearly always team-work. It's very, very seldom that an engineer gets a project like Tom Swift or some of those comic books where you've got this one hot-shot engineer who does everything. Engineers are taught to work as a team all the way through school. In every project that they work on, they work as a team, and if you want to get a good grade on a project in school, you have to do 80 percent of the work, because the other people may not care that much about having a good grade. So if you want a good grade, you've got to put forth effort. When you get out in the real world, it's the same kind of thing. You have to go a little bit more than your fair share on a project in order to ensure the success of the project.

A chemist or a physicist, they operate in a little bit different environment. They've been taught all the way through school that you can't copyright something, that you can't patent something in those areas. The way that you get well known is you write something down and present it at a conference, and once you present it at a conference, you get credit for it, and your standing in the world depends on how many of these things that you take credit for. You can nearly always tell when somebody's arrived. You just go to these conferences and look at the publications or the index, and when you start referencing more of your work than you do of somebody else's work, you consider that you've arrived. Up until that time, you're riding on the coattails of somebody else.

That's probably about all I can tell you about this.

BERGEN: Well, before we close I'd like to see if Tim and Kevin have any questions for you. Tim?

FARRELL: I can't think of any others.

BERGEN: Kevin?

RUSNAK: Yes, I have a few. You mentioned earlier, you were talking about the difference in attitude among engineers here from Apollo to Space Station. How would you say the Space Shuttle fit in that transition?

POHL: When the Space Shuttle came about, that's what I tried to get across, about we had just powered down the Center. We had laid off lots and lots of people here, government workers as well as contractors, and then from the Apollo Program we had taken a big decrease, a big hit. The people that was left here when the Shuttle was started had the feeling that if they did not build that Shuttle within the schedule and within the cost, there would be no agency, there would be no NASA. So people really worked on Shuttle out of the feeling that they wanted to preserve the agency, and they wanted the Shuttle to be successful because they felt like there would be no agency if the Shuttle was not successful. So I think that was a big change.

In Apollo, it was the competition with the Russians. On Shuttle, it was the protection of the agency, and then on Space Station, it was more thinking about "my job" and "my responsibility" and "my authority to direct somebody else to do something." Nobody wanted to do anything on Space Station, but everybody wanted to tell somebody else what they had to do on Space Station, and that was a big change that I've seen between those programs.

RUSNAK: While you were working on Shuttle development—I'm particularly thinking early on in the early 1970s—how much did budgetary considerations affect or limit your design capability?

POHL: We had a major, major problem with the budget. Not that we didn't have enough resources; it was that we couldn't use our resources very well. The problem was on Shuttle, was they'd give us so much money this year, and next year they'd promise you that much. So you would plan to gear up and do this. Well, when next year came, they gave you this amount again and the next year you're going to build up and get all this money. Every year you was going to get well next year, but that never happened.

Well, we would plan on and gear up for the people and things to do what was proposed in the budget, only to have that cut and slid out. The Shuttle was—you know, there's lots of ways of looking at things, but if you factor out inflation on Shuttle, it came in just about what was proposed. There was very little overrun, what I would call as pure overrun on the Shuttle. It probably came in as close to budget as any program we've ever worked on, at least the Orbiter part of it did.

I remember about the same time the contract was let for the Shuttle, the contract was let for the B-70, I believe it was, and they were going to build five B-70s. Minus the avionics on the B-70, the cost of development of the B-70 was about the same as the cost of development of the Orbiter. Now, if you look at the technologies that had to come to bear on the Shuttle versus the technologies that you had to develop for B-70, there was no comparison. The B-70 was just another airplane, a little faster than the previous one, but it was a big, big bomber. The Shuttle had to go into space, it had to come out of space.

You know, you think about kicking the Shuttle out of orbit halfway around the world and landing it on this little two-mile strip down here with no engines, you had to know the drag coefficients on that airplane all the way down through all the mach numbers, and that all had to be developed and all had to be confirmed in wind-tunnel testing. You know, hundreds of thousands of hours of wind-tunnel testing was done on that vehicle on different models, every size. I guess we used every wind tunnel in the United States to do some testing, because we didn't trust the data we would get from one. You had to confirm it in another one and another one this size and that size, and you're working with these little old-scale model things, and then you have to scale it up to full scale and try to figure out the scaling factors. Not everything scales linearly.

So that was a big problem. We wasted a lot of money in the Shuttle Program simply because of the way the budget process worked and not getting the money on the years that it was promised when it was promised.

RUSNAK: How would you describe the relationship between your branch and later the division and the Space Shuttle and Orbiter Program offices?

POHL: Oh, as long as Aaron Cohen was Shuttle program manager, project manager, we got along very, very good, and I got along with Warren Morrison and Bob Thompson real well. I think the people in the division was highly respected by the program, project management in that time frame.

RUSNAK: A specific question on Shuttle. Was there any consideration of using an all-electric fly-by-wire system for the control surfaces instead of hydraulics?

POHL: Yes. Matter of fact, that's one of the battles we lost, and I thought I had it sold and dropped the ball on it. Yes, we designed an all-electric vehicle. I didn't like hydraulics. I just knew that sooner or later we were going to have a hydraulic leak and sooner or later we were going to have a fire in the back of that vehicle when we come home because we got all that hydraulic fluid back there, and it's going to get hot enough, it's going to catch fire and we're going to have a problem.

So we wanted to go with an all-electric system, and we pretty much sold that all the way up through Bob Thompson here, but then we went out to Rockwell and made a presentation out to Rockwell, and I guess George [W.] Jeffs made the comment that he was going to have to lay off 200 hydraulics people, and he was going to have to hired 200 electric people, and Dr. Kraft says, "Forget it." And so he made the decision not to change.

They still have work going on out there now, studying and looking at those things, and we've made a lot of progress in those kinds of systems. A lot of fighter aircraft now have all electric airplanes, because you shoot holes in hydraulic lines and it's kind of unforgiving. You can put two wires in there and you shoot a hole in one wire, and it don't affect the other wire too much.

But we have the capability now to build fuel cells where we could do away with the APUs, we could do away with the hydraulic systems, and go with an all-electric airplane that would probably be more reliable and safer than what we've got, and maybe one of these years they'll start upgrading and going with some of the technology. It'd save a lot of weight and give you more payload. Now you go into Space Station, you need every bit of payload you can get because that's costing us about 12,000 pounds to go to 57 degrees.

RUSNAK: Last time you talked a little bit about Guy [Joseph G.] Thibodaux from propulsion and power. I wanted to ask you about someone else who was in the same division, Chet [Chester A.] Vaughan.

POHL: Chet Vaughan. Chet Vaughan and I worked together from the day I walked into this Center. If you want something done, you give it to Chet Vaughan. There is no job too big, there is no job too little for Chet to do, and he's just like a little tiger when he gets a hold of it, he won't let go until it's done. Extremely conscientious guy with an enormous amount of energy. If coffee needs to be made, he'll make coffee. If you've got this big management problem, you can give it to him and he'll wrestle with it until he gets it fixed. Give him a design problem, he'll wrestle with it until he comes up with the answer to it. A tremendous guy.

You know, we were very, very fortunate there in the division of having very good, extremely conscientious people. We just happened to have picked up a good group of people. I was kind of discouraged when I first came down here and started working with Mr. [Aleck C.] Bond and Mr. [Joseph N.] Kotanchik. Matter of fact, I even almost went back to Huntsville, because I had come from an organization that gave us a lot of responsibility, didn't tell us ho to do anything except they wanted it done yesterday, and then I come down here where we'd go to a staff meeting and spend four hours in a staff meeting worrying about editing reports, who the editor ought to be and what kind of style we ought to use, and if something got in Aleck's briefcase, it just didn't get out.

He was used to this research atmosphere at Langley where your product was the report and you design something, you build it, you put it in a wind tunnel. Most people don't realize it, but every airplane in World War II flew with a Langley wing. Langley never built an airplane in their life, but they developed the criteria for a very efficient airfoil for the wings. Same way with Lewis. Lewis never built an engine, but every airplane that flew, flew with the cowling that was developed at Lewis for the air flow to go through and cool the engines. So those people came more from a research side of the house. When they first started out on Apollo Program, they couldn't quite break away from that mold, and I found that very, very difficult to work with. I'd get so aggravated and frustrated with those two guys.

Fortunately, Guy Thibodaux came down and took over the division in about a year after I got there, I guess, and Guy was just the flip side of these other two guys. He was extremely bright. I don't know of anybody that knows more about more things than Guy Thibodaux. I never did find a guy really coming up with something that was not right. I do remember him telling Joe Shea something when he first came down here one time that I didn't think was right, and I waited until he got back in his office, walked in there, and said, "Guy, I need to talk to you."

"Come on in." He was always very gruff and very intimidating.

I told him, I said, "Well, you told Dr. Shea such and such, and I'm of the opinion that that's not right."

His eyes got cold. He looked me right in the eye and said, "You and I need an understanding here and now. If I ever say something, if I ever do something that you think is not right, you tell me then and there that you think it's not right, and you tell me why you think it's not right, and you listen to my comeback, and then you come back on my come back to convince me where my comeback is faulty." And he says, "If you still think I'm wrong, you go back down to your office, and you write down all of the reasons on a piece of paper why you think that decision is wrong, and you bring them in here, and you sit down with me, and you got over them with me one by one. And if in three tries you haven't convinced me that I'm wrong, you've got two choices. You can get down to your office and do it my way, or you can quit." And that was some of the best advice I ever got, and Guy practiced that.

Then he just went over and picked up the phone and called Joe Shea and says, "Henry's in here, and Henry just reminded me that I told you something that was incorrect," and told him what it was. Most people, when they say something that's not right, they hope it'll go away without telling anybody, without having to admit that they made a mistake, but not Guy. He was just up front with it, and that's the way I found him with everything. Not one time did he ever stray, give me bad advice, or steer me in the wrong direction.

I remember I had a problem with this one guy out there. I couldn't supervise him. He did a super good job on the things he wanted to work on, but he wouldn't work on things I wanted him to work on. I went in there one day and told Guy, I says, "I've got a problem with So-and-so, and this is the problem."

He says, "Henry, you've got a problem. It's not him, it's you." He says, "You're trying to change him." He says, "Don't ever try to change somebody." He says, "My professional experience tells me that every human being can do something good, and it's a supervisor's responsibility to find out what that is and use him in that area, and if you can't use them in that area, get rid of them, but don't try to change them." Very good advice. I followed that all of my life, followed that philosophy.

A lot of the things that I picked up, a lot of the characteristics that I picked up, I picked them up from Guy Thibodaux. Matter of fact, he probably did more for my success and my advancement through the organization than anybody else because he laid down a foundation, he laid down the work ethic of honesty and integrity that was absolutely beyond reproach. He had enough self-confidence in himself that he didn't have to prove something to somebody else. He would let the people have credit for something when credit was due. He was not anxious to get any credit for anything himself.

I ought to bring you an article I wrote for his retirement, if I can still find it. I know Guy's still got some copies of it. I sat down and wrote something out the night before his retirement that I read up there about comparing him with Julius Caesar and Mark Anthony and those people. That's where he found out that I knew something about Shakespeare. I'll see if I can't find one of those articles and send it to you.

BERGEN: That would be great.

RUSNAK: Just one final question. You did a chapter on reaction control systems in the manned spacecraft engineering and design.

POHL: Yes.

RUSNAK: Do you recall the circumstances behind that book and your involvement with it?

POHL: Well, that's the brainchild of Paul [E.] Purser. When we first moved down here, there was no good text anywheres on the design of spacecraft, and we knew we were going to have to start hiring a bunch of young people out of college. What Paul wanted was some kind of a text that could be used for the engineers, and particularly the engineers at Rice University, because we thought we was going to hire a bunch of those students and teach them a little bit about spacecraft design. So the intent of that book was to cover all of the disciplines that was required to design spacecraft, manned spacecraft, and so you've got a chapter in there on each one of those disciplines.

Well, I happened to have the RCS at that time, so it was my job to write that chapter. I didn't spend a lot of time on it. That was done mostly at night at home, because days were pretty much taken up with the day-to-day activities, so I'd go home at night and I probably worked on it two or three nights at home, and somebody typed it up and put it in the book, and that's it.

When I first came down here, we needed some test facilities, and I made the mistake of telling Dick [Richard B.] Ferguson, "Well, I can design you a test facility."

"Would you?"

So I go home at night and I design one, just sketch it out on a piece of notebook paper, and took it back in and sat down with him and explained it to him, thinking he was going to go give it to an architect and they were going to put it in all the drawings and everything. Shoot, they put it out on bid and built it just from that sketch from the notebook paper. In two weeks' time, we had a test facility out there in Ellington Field. That building might still be standing out there, because I think they made it out of six-inch-thick reinforced concrete, so if something blew up it wouldn't hurt you.

RUSNAK: All right. That's all I have. Thank you.

POHL: Okay.

BERGEN: Do you have anything you'd like to say in conclusion?

POHL: Well, I guess not. The one thing I will say is that I was extremely, extremely fortunate in having an opportunity to work in an environment at a time and a place where it was fun to get up and go to work every single morning. I was raised on a farm. I really wanted a farm, but I couldn't make enough money farming to buy a farm. Engineers was getting this huge salary. They were getting $400 a month at that time. So I was going to go four years to college, get a degree in engineering, then I was going to go work for four years, and I was going to make enough money in those four years to buy a farm.

Well, when I graduated, I got drafted in the Army, did my basic at Fort Bliss [El Paso, Texas], then I went out to Huntsville and got to see my first rocket engine. I lost all interest in farming then. That's what I wanted to do, and that's what I've done. You know, I am convinced that our contributions that we made in our lifetime has made the quality of life of humans a whole lot better than it would have been had we not had the opportunity to work on the kinds of things that we worked on. It's been fun. You know, a lot of people go out and work and make money to go do the things that they like to do, like playing golf. I never did take up golf because anytime I wasn't working, I was either thinking about it or I was out at the office. I was either home or I was at the office. That's the only things that mattered. There was nothing else.

I remember being in Huntsville on test day one Saturday night. I have to tell you this one real quick, and maybe I did. But this technician came up to me—I was in the Army—and says, "Henry, they can't make you stay out here at night. I'd just tell them I'm not going to do that. You ought to be downtown having a good time, single, young, and in the Army. You ought to be downtown having a good time."

And you know, I couldn't figure out how I could tell this guy that there was nothing in this world that I wanted to do worse than what I was doing that night out there on that test stand. That was a whole lot more fun than it would have been carousing around town or boozing it up someplace. And that's kind of the way my life has been all the way through. It's been something that I wanted to do more than anything else. So I never did mind putting in the extra hours and extra time. I kind of enjoyed it. That was my interest.

BERGEN: It's wonderful that you can look back over your career and be glad that you did what you did and you made a contribution.

POHL: Yes, and I keep telling everybody that I don't want to do it over again because I'd just screw it up worse the next time.

BERGEN: Well, we thank you so much for sharing your memories with us. We've really enjoyed hearing about it.

POHL: Okay. Well, if I can help you out any other way, let me know, anytime.

[End of interview]